Former theories of the general circulation

Before considering the manner in which the balance requirements now appear to be fulfilled, we shall examine some of the views which prevailed in the past. So-called theories of the general circulation, whether they were real attempts to account for the circulation by dynamical theory, or merely descriptive schemes unaccompanied by explanations, appeared in abundance during the nineteenth and early twentieth centuries. Bergeron (1928) even remarked that there were as many theories as authors. We cannot discuss or even mention the great majority of these, but we shall attempt to identify those ideas which most greatly influenced the subsequent development of the subject, and which have led us to our present state of knowledge.

It is a relatively easy matter today to determine whether any newly proposed scheme of the general circulation agrees in its main features with observations, and to discard the scheme if it does not. In judging the worth of an older theory, we must therefore recall that much of what we now look upon as the observed circulation was unobserved as recently as World War II, and that at the close of the nineteenth century even such familiar entities as the stratosphere had not been discovered. Thus the main features of some of the former schemes were their speculations as to the circulation in the regions where observations were not available.

If the circulation were uniquely determined by the governing laws, any proposed scheme later found not to agree with the newer observations would necessarily violate some dynamical principle. We must therefore note that there may be many different circulations which satisfy the dynamic equations. Moreover, even if the external conditions should determine the circulation uniquely, considerably different circulations might be properly deduced from slightly different assumptions concerning the external conditions; this possibility has been cited by Bergeron (1928) as a contributing factor to the abundance of theories.

We should therefore regard a theory as a worthy contribution to the knowledge of its time if it contains no flaw in its dynamical reasoning, and if it is consistent with the observations available when it was formulated. A necessary condition for a theory to be dynamically sound and also compatible with observations is that it satisfy the balance requirements demanded by these observations. The condition is not sufficient; a proposed circulation may transport the proper amounts of angular momentum, water, and energy across each latitude and still be deficient in other respects. Noting this possibility, we may yet judge the worth of a proposed scheme partly by its ability to satisfy the balance requirements.

The circulation pictured by Hadley (1735), discussed in detail in the first chapter, satisfies the balance requirements demanded by observations which were then available, although not all of the requirements which more recent observations demand. The upper current carries as much mass poleward as the lower current carries equatorward. It carries more angular momentum, since the westerlies aloft are stronger. It also carries more sensible heat and internal energy, if the stratification is stable.

Hadley's scheme does not contain the weaker equatorward flow of angular momentum at high latitudes, but neither does it contain the polar easterlies which would demand it. Hadley did not consider water, but presumably his circulation would carry more water equatorward than poleward across every latitude, yielding the equatorial excess of precipitation and perhaps the deficit in the subtropics, but also giving a deficit in the polar regions, in contrast to what is observed.

Figure 30 illustrates Hadley's circulation schematically. Hadley himself presented no figure; we have introduced Figure 30 for comparison with the figures which have accompanied the numerous subsequent works.

In Hadley's picture the horizontal transports needed to satisfy the balance requirements are accomplished by the simplest possible mechanism — a meridional circulation where the uniform poleward current at one elevation carries a different amount of each transported property from the uniform equatorward current at another elevation. Since the atmosphere is not constricted to behave independently of longitude and time, other mechanisms are available. Whenever large-scale eddies such as cyclones are present, poleward flow at one longitude is accompanied by equatorward flow at the same time and elevation at another longitude. The oppositely directed currents may carry different amounts of any property. Theories of the general circulation therefore conform to one of two general schemes — those in which eddy motions are either absent or irrelevant, and those in which the eddies influence the zonally averaged motion by transporting some property from one latitude to another.

Following the publication of Hadley's paper there was a period of more than half a century during which the scientific community was generally unaware of its existence. Similar explanations were even rediscovered by such savants as Immanuel Kant (1756) and John Dalton (1793). Later Dalton encountered Hadley's paper, and, in acknowledging that his own contribution had been completely anticipated, noted

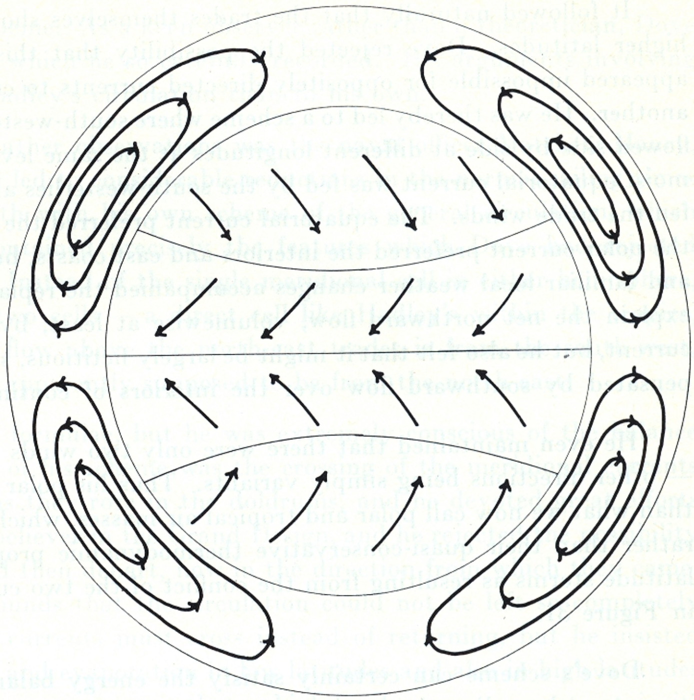

Figure 30. — A schematic representation of the general circulation of the atmosphere as envisioned by Hadley (1735)

the tendency for current works to continue to quote the older inadequate theories, while continuing to ignore the more recent and more acceptable ones. His remarks remain true today.

In due time, however, Hadley's explanation became the one which was quoted in the standard works. Early nineteenth century observations being what they were, there was little reason to doubt the explanation in its essential features.

As the nineteenth century progressed, observations began to cast some doubt upon Hadley's scheme. In particular, there was growing evidence that the prevailing westerlies in the northern hemisphere tended to blow from somewhat south of west, instead of from somewhat north of west as Hadley's explanation would have demanded. Undoubtedly the available data did not really justify such a conclusion, as they were confined largely to oceanic regions. Nevertheless the conclusion was evidently correct, as indicated by today's vastly more complete observations.

With the realization that the surface separating the trade winds from the south-westerlies above them sloped downward toward the north, and reached the Earth's surface in the horse latitudes, the notion became established that the middle-latitude south-westerly current at the surface was simply an extension of the current above the trades. The question then arose as to how the air returned from higher latitudes.

An answer was provided by the eminent meteorologist and climatologist Heinrich Wilhelm Dove (1837). Earlier (1835) Dove had been one of those to rediscover Hadley's explanation of the trades, again under the assumption that absolute velocity rather than absolute angular momentum would be conserved in the absence of east-west forces. He now accepted Hadley's ideas completely as far as low latitudes were concerned, but he favoured the prevailing idea that the south-westerly winds in middle latitudes were a continuation of the south-westerlies above the trades, since he felt that their warmth and humidity demanded an equatorial origin. At that time it was not generally realized that air rising to high levels and sinking again would have to lose most of its moisture.

It followed naturally that the trades themselves should be a continuation of a return current from higher latitudes. Dove rejected the possibility that this current could occur at upper levels, since it appeared impossible for oppositely directed currents to cross in the horse latitudes without altering one another. He was thereby led to a scheme where south-westerly and north-easterly winds in middle latitudes flowed side by side at different longitudes at the same level, rather than one above the other. His warm moist equatorial current was fed by the south-westerlies above the trades while his cold dry polar current fed the trade winds. The equatorial current preferred the oceans and the west coasts of continents, while the polar current preferred the interiors and east coasts, but the longitudes of the currents were not fixed, and familiar local weather changes accompanied the replacement of one current by the other. He could explain the net northward flow, volumewise at least, by the greater specific volume of the equatorial current, but he also felt that it might be largely fictitious, since the observed northward flow could be compensated by southward flow over the interiors of continents, where observations were less abundant.

He even maintained that there were only two winds in middle latitudes — the north and the south — other directions being simply variants. Thus his polar and equatorial currents seem to be none other than what we now call polar and tropical air masses, which he chose to identify by their preferred motion rather than their quasi-conservative thermodynamic properties. He furthermore regarded the middle-latitude storms as resulting from the conflict of the two currents. His circulation is shown schematically in Figure 31.

Dove's scheme can certainly satisfy the energy balance requirements, since the equatorial current is warmer than the polar current. It can satisfy the momentum balance requirements, since the south-westerly winds carry more momentum than the north-easterlies. Under the assumption that the equatorial current cannot carry its water aloft at low latitudes, as Dove had supposed, but must lose it and then reacquire it from the ocean after descending, the scheme can satisfy the balance requirements for water.

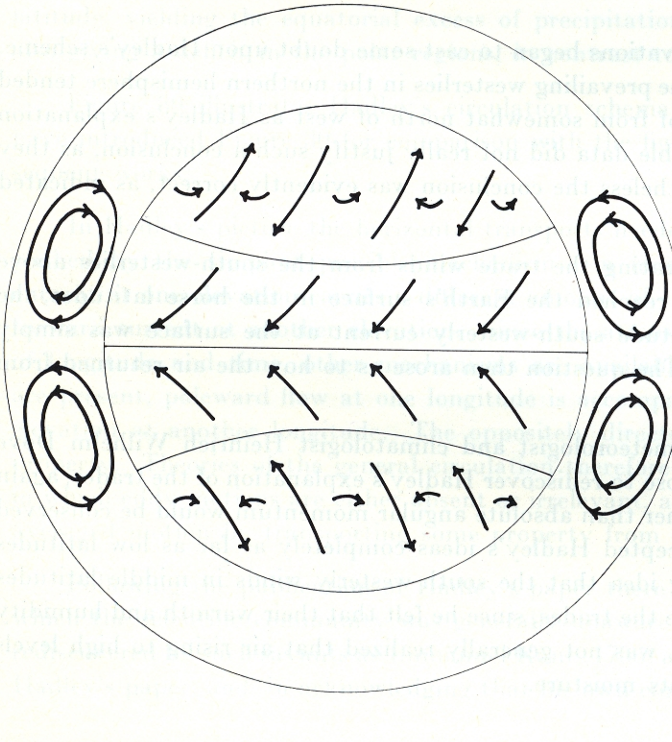

Figure 31. — A schematic representation of the general circulation of the atmosphere as envisioneed by Dove (1837)

Yet it does not have the appeal of Hadley's scheme. As a keen observer rather than a theoretician, Dove offered no tidy explanation for the circulation which he so carefully recorded. His arguments involving the Earth's rotation are more applicable to Hadley's circulation than to his own.

Among the most diligent compilers of weather observations was the naval officer Matthew Maury, whose charts of the winds over the oceans had led to considerable reductions in the normal sailing times between distant points. In 1855 he came forth with his own scheme of the general circulation, which departed considerably from Hadley's, and contained precisely the features which Dove had rejected some years before. It is shown in Figure 32. Instead of the single meridional cell in either hemisphere, or opposing currents side by side, there are two cells — a direct cell like Hadley's within the tropics, and an indirect cell in higher latitudes. The flow above the north-east trades is from the south-west, and the upper-level flow at higher latitudes is apparently supposed to be from the north-east.

Like Dove, Maury used no mathematical formulae, but he was extremely conscious of the balance requirements for water. A distinctive feature of his scheme was the crossing of the meridional currents as they sank in the horse latitudes and also as they rose in the doldrums, and he devoted great efforts to justifying these crossings. He was a great believer in the Grand Design, and he rejected the possibility that the converging currents would mingle and then depart, now in the direction from which they came, and now in the opposite direction, on the grounds that the circulation could not be left so completely to chance. He could see no reason why the currents must cross instead of returning, but he insisted that the lack of balance between precipitation and evaporation in low latitudes and also in high latitudes indicated that they did cross. Like Dove, he was unaware that a high-level current cannot retain its moisture. He believed that crossing without mixing could occur by having vertical columns of air pass one another; his envisioned columns seem to have the dimensions of cumulonimbus towers.

Maury was unable however to offer an explanation for what seems now to be the principal feature of his scheme — the indirect cells in higher latitudes. He accepted Hadley's explanation for the trade-wind cells, and simply said that the cause of the indirect cells had not been explained by philosophers.

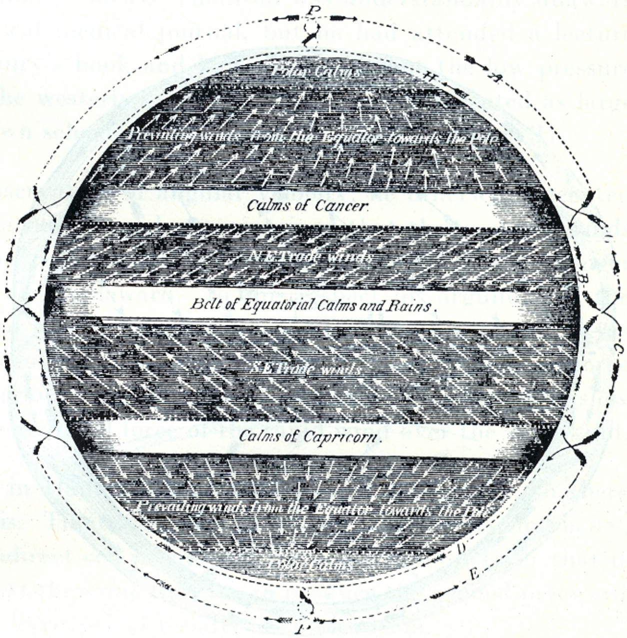

Figure 32. — The general circulation of the atmosphere according to Maury (1855)

Maury's scheme seems to satisfy the balance requirements for angular momentum, in view of the assumed crossing currents in the horse latitudes and the upper-level easterlies in higher latitudes, which, however, are inconsistent with modern observations. It certainly cannot satisfy the energy balance requirements, except for an atmosphere which is heated at the Equator and the Poles and cooled in between. Nevertheless, Maury's book is extremely readable. It became rather popular in his day, and it was instrumental in initiating some of the more rational theories which were to follow.

Among those who read Maury's book was the school teacher William Ferrel, whose interest in the subject was thereby aroused. Here he first learned that the normal pressure was not uniform over the Earth's surface, but highest in the horse latitudes, and lower in the doldrums and especially in the polar regions. He found that he disagreed with some of Maury's ideas, particularly the crossings of the meridional currents, which he felt ought to mix rather than cross. In the following year (1856) he came forth with a theory of his own.

The circulation which he envisioned is shown in Figure 33. It is somewhat like Maury's, except that there are now three cells in either hemisphere, which he felt were demanded by the observations. Unlike Maury, however, Ferrel believed that he could present a complete explanation.

Ferrel's great contribution in this paper was the introduction of a "new" force; the north-south component of the Coriolis force, which he incidentally identified with one of the terms in Laplace's tidal equations, formulated long before Coriolis, and which he believed had not been previously recognized in any meteorological work. Actually he had been anticipated in an unnoticed paper by Tracy (1843), who with inadequate arguments nevertheless deduced the proper direction of the deflection. Ferrel believed that proper consideration of the new force would account for the previously unexplained features not only of the general circulation but of cyclones and smaller disturbances as well.

Ferrel agreed with Hadley that the prime moving force of the atmospheric circulation was the Pole-to-Equator density gradient brought about by solar heating, which he believed should lead to meridional

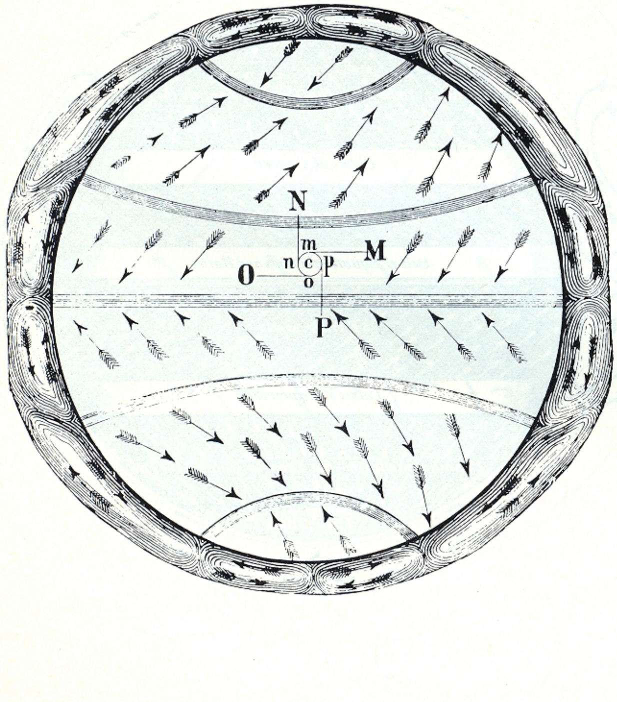

Figure 33. — The general circulation of the atmosphere according to Ferrel (1856)

motions, and hence, through the action of the east-west Coriolis force, to easterly and westerly winds distributed much as Hadley had supposed; but he did not find in Hadley's theory any explanation for the distribution of pressure. He then noted that through the action of the new force the easterlies in low latitudes and the westerlies in higher latitudes should be deflected away from the Equator and the Poles toward the subtropics, thereby creating the observed deficit of pressure at the Equator and the Poles and the excess in the subtropics. To explain the poleward drift in the surface westerlies, he observed that because of surface friction, the winds near the ground would be considerably weaker than the winds somewhat higher up, while the horizontal pressure gradient would be reasonably uniform. The southward Coriolis force near the ground would therefore be insufficient to balance the pressure gradient, and the westerlies would turn poleward, later to rise and return equatorward.

Ferrel also noted that for hydrostatic reasons the latitude of highest pressure must be displaced toward the Equator with elevation. He apparently felt that the opposing currents aloft must meet at this latitude in order to maintain the high pressure, whence he showed inclined boundaries between the low-latitude and middle-latitude cells.

There are certain obvious deficiences in Ferrel's scheme, as well as in his explanation of it. The indirect cells in middle latitudes must transport angular momentum and energy toward the Equator, and neither balance requirement can be satisfied. The middle-latitude westerlies aloft were originally supposed to be maintained by the action of the Coriolis force upon the poleward currents, but now these currents have been replaced by equatorward currents while the westerlies are allowed to remain.

Nevertheless it would be difficult to overestimate the importance of Ferrel's paper. Here he first presented to the meteorological world a correct account of the Coriolis force, a quantitative description of the geostrophic wind, and a partial explanation for its occurrence. His demonstration that the pressure field could adjust itself to fit the wind field, rather than forcing the wind to do all of the adjusting, has often been overlooked by succeeding generations of meteorologists.

Another scientist who read Maury's book was the physicist and inventor James Thomson, who found himself in considerable disagreement with some of Maury's ideas. Thomson was understandably unaware of Ferrel's work, which had been published in a local medical journal, but he had attended a lecture delivered by Murphy (1856), who had also read Maury's book and had suggested that the low pressure at the Poles resulted from the centrifugal force of the westerly currents, which could be treated as large circumpolar vortices. Thomson soon produced his own scheme (1857), which is shown in Figure 34.

After noting Hadley's error concerning the conservation of angular velocity, he otherwise accepted Hadley's arguments with regard to the bulk of the atmosphere, but maintained that the westerly winds near the ground, being slowed by friction, would possess a deficit of centrifugal force relative to the stronger westerlies immediately above, and would therefore drift poleward. In this respect his argument is the same as Ferrel's, differently worded. The southward or northward component of the Coriolis force, as Ferrel pointed out, is simply the excess or deficit of centrifugal force as compared to the centrifugal force of a particle rotating with the Earth. The excess Coriolis force of a rapid west wind over that of a slow west wind is therefore the same thing as the excess centrifugal force of the rapid wind over the slow wind.

Just as there was little observational evidence in Hadley's day to contradict his scheme, so there was little evidence in Thomson's day to contradict his. Thomson's scheme is admirable for its simplicity, and it also satisfies the balance requirements. The indirect cell is confined to such a shallow layer that it transports very little angular momentum or energy. At the same time it can produce the needed poleward transport of water, since the water-vapour content decreases so rapidly with elevation.

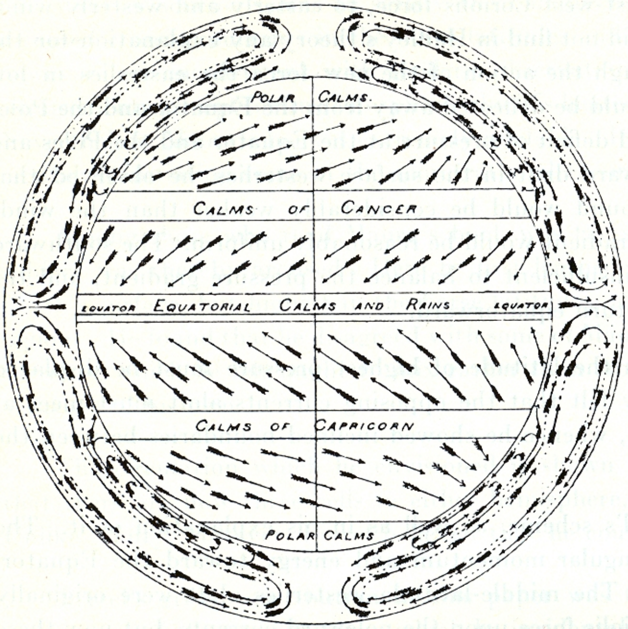

Figure 34. — The general circulation of the atmosphere according to Thomson (1857, 1892)

Thomson's published contribution was limited to an abstract, but years later (1892) in his Bakerian Lecture, delivered two months before his death, he returned to the problem of the atmosphere, and reiterated his belief in his former scheme. He also discussed critically the other schemes which had been offered, and noted the difficulties entailed by Maury's and Ferrel's converging currents aloft.

Ferrel was, however, a relative newcomer to the field, and his ideas were anything but static. He set about formulating his work in mathematical terms, and as result he came up with a revised scheme (1859). It is shown in Figure 35. It is very much like Thomson's, except in the polar regions where definitive observations were lacking in any case. It is therefore equally effective in fulfilling the balance requirements. His paper contains the complete equations of motion for the atmosphere, and an account of the thermal wind relation.

In justifying his scheme, he maintained that if surface friction were absent, while internal friction still existed, the atmosphere would assume a condition of uniform absolute angular momentum. It is often pointed out that such a condition would lead to unrealistically violent winds at high latitudes, but Ferrel went a step beyond his successors and noted that the accompanying pressure gradients required by geostrophic balance would leave the polar regions completely devoid of air. In his computations he had treated the atmosphere as a liquid; as a gas the only real singularities would be at the Poles, but even between 30° and 60° latitude the pressure would drop by a factor of three.

Ferrel maintained that with surface friction the atmosphere would tend toward the same distribution, but to a much lesser extent, the latitudes separating the easterly and westerly surface winds being ultimately determined by the requirement of no net surface torque. He thus purported to account for the observed distribution of zonal wind. He explained the poleward drift of the surface westerlies as in his earlier paper, observing that this drift required a return current somewhere aloft. He noted, however, that in view of the thermal wind relation, the upper-level westerlies must be stronger than the

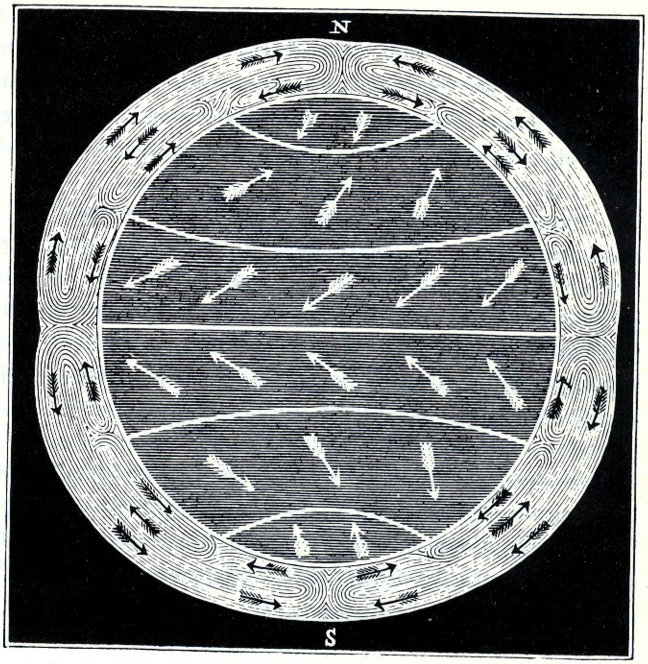

Figure 35. — The general circulation of the atmosphere according to Ferrel (1859)

surface westerlies and must be maintained by the action of the Coriolis force upon a poleward current. He therefore placed the equatorward current at an intermediate level, noting that according to observations it should lie above the fair-weather clouds.

We cannot agree with Ferrel's premise that with internal viscosity but without surface friction the atmosphere would tend to acquire a state of uniform absolute angular momentum. Such a circulation would possess strong internal stresses. Neither does it seem very likely that the ultimate circulation with surface friction would be an attenuated form of the circulation without friction, despite Ferrel's observation that there can be no resistance to motion until there is motion. Whereas Thomson by-passed an explanation of the surface easterlies and westerlies by simply agreeing with Hadley, Ferrel's attempt in this respect yielded no improvement. Beyond this point, Ferrel presented some penetrating arguments, and he used the thermal wind relation to good advantage.

Ferrel's subsequent work led to further modifications, his final scheme (1889) differing slightly from his second one. He was intrigued by the possibility of deriving mathematical expressions for the circulation, but felt that this could not be done because the frictional forces could not be properly formulated. He continually maintained that the circulation must be derived from a knowledge of the temperature field, rather than the field of solar heating, and his system of equations does not contain the thermodynamic equation. It was a great loss to nineteenth-century meteorology that the man who introduced the equations of motion never saw fit to seek a complete solution of them.

The task which Ferrel had regarded as unfeasible was finally attempted in a pair of papers by Oberbeck (1888), who represented the effects of friction by a simple coefficient of viscosity. Like Ferrel, Oberbeck sought to derive the motion from the temperature field, and he did not use the thermodynamic equation. He represented the temperature by a simple analytic function of latitude and elevation.

Oberbeck sought first the circulation which would prevail in the absence of rotation and advection, and the set of equations which he first solved expressed a balance between the effects of friction and the pressure forces. The circulation which he obtained was necessarily entirely meridional, and consisted of a single direct cell. To obtain the next approximation he balanced the east-west Coriolis force, as determined by his first approximation, against friction. The added circulation was entirely zonal and proportional to the Earth's angular velocity Ω, and consisted of low-latitude easterlies and high-latitude westerlies at low levels, and westerlies at all latitudes at high levels.

On the whole his circulation bears considerable resemblance to Hadley's. We feel, however, that this resemblance is fortuitous. In a steady symmetric circulation, the Coriolis force resulting from the net north-south motion in any vertical column is zero. Hence Oberbeck was balancing the frictional drag at the base of each column against the net Coriolis force resulting from the weak vertical currents, and he thus obtained easterlies and westerlies just above the surface, in their proper latitude. In a mathematical description of Hadley's circulation, the frictional drag is balanced by non-linear terms, which Oberbeck had not used at this point.

In his second paper Oberbeck sought the final corrections needed for an exact solution, but since the system of equations governing these corrections was as complicated as the original system, containing all the non-linear terms, he found it necessary to make further approximations. The added circulation was again entirely meridional, and proportional to Ω^2, and consisted of a direct cell in low latitudes and an indirect cell in high latitudes. In essence he had found the first three terms of a power series in Ω. For the value of Ω appropriate to the Earth, the added cell in high latitudes was insufficient to reverse the direction of the original cell, and simply weakened it there while intensifying it in low latitudes.

It is no discredit to Oberbeck that he was forced to stop with the quadratic terms in Ω, yet it must be conceded that on this account his solution is not a particularly good approximation to the exact solution which he sought. His increase of westerly wind speed with height is proportional to Ω, whereas, according to the thermal wind relation, it should be inversely proportional to Ω. It is not at all certain that Oberbeck could have improved his results by computing more terms, since, as noted by Brillouin (1900), there is no assurance that the series would converge. A power-series expansion does not reveal that $\Omega/(1 + \Omega^2)$, for example, becomes small as Ω becomes large.

Oberbeck's work marks the beginning of a new field of endeavour — representing the global circulation by solutions of the dynamic equations, as opposed to using the equations simply to deduce general properties. A mathematical solution is simply one type of description of the circulation, but its advantages are obvious. If the equations have been correctly formulated and correctly solved, with no crippling approximations, the description is assured of being internally consistent in every way, and in particular it will satisfy its own balance requirements.

If more theoretical meteorologists had followed Oberbeck, and had sought actual solutions of the dynamic equations in preference to circulations which could merely be rendered plausible by qualitative arguments based upon the dynamic equations, many of the impossible schemes which were subsequently offered might never have appeared. Still, the idea that manipulation of mathematical symbols ought to replace qualitative reasoning could scarcely have appealed to the many competent meteorologists who were nevertheless not mathematically inclined. When further attempts to solve the equations yielded circulations which were no more realistic than Oberbeck's, this was cited as evidence that the whole procedure was meaningless. The fact that the equations had not really been solved was disregarded.

It must be admitted that even very recent analytic solutions of the equations have had a certain unrealistic flavour. It is only with the advent of numerical solutions by digital computers that the equations have begun to acquire the status which they deserve.

In Oberbeck's work, as in that of most of his predecessors with the notable exception of Dove, the general circulation was treated as being completely symmetric with respect to the Earth's axis. It must not be supposed on this account that the various authors were unaware of the presence of cyclones and other disturbances. Both Ferrel and Oberbeck were deeply concerned with the cyclone problem, and Ferrel often dealt with the general circulation and cyclones in separate sections of the same papers, regarding the cyclone circulations as being much like the general circulation on a smaller scale. Yet nowhere in Ferrel's work is there any suggestion that cyclones owe their origin or subsequent behaviour to the general circulation, or that the general circulation in turn is affected by the presence of cyclones.

The idea that storms were dependent upon the general circulation had been proposed long before Ferrel's time, and it formed an essential part of Dove's work. In modern studies where the field of motion has been analysed into "zonal" and "eddy" components, there is often a tendency to regard all departures from zonal symmetry as having a similar nature, and to refer to them loosely as storms. As an observer of weather phenomena rather than a formalist, Dove distinguished between cyclonic storms on the one hand and the equatorial and polar currents on the other, regarding the storms as originating from disturbances of the opposing currents. It would have meant nothing to him to inquire whether these currents influenced the general circulation; to him they were the general circulation. Indeed, such a question is meaningful only if the general circulation has been defined. A less ambiguous question would ask whether the zonally averaged motions are different from what they would be if departures from zonal symmetry were absent. Dove might have answered this question in the affirmative.

It is remarkable that Dove's rather advanced description of the circulation, which would have been dynamically possible in a dry atmosphere, went completely unmentioned by many subsequent writers (for example Brillouin, 1900) who included thorough treatments of the works of Maury, Ferrel, Thomson, Oberbeck and others in their historical discussions of general-circulation theories. Among those who did mention Dove, Waldo (1893), while presenting extensive reviews of the other works, merely states that Dove made certain modifications of Hadley's theory; he does not even say what these modifications were.

Perhaps the neglect of Dove's work may be traced to his refusal in his later years to accept any of the newer ideas, with the result that all of his work tended to become discredited. Perhaps his work was ignored because he offered only descriptions rather than explanations, although the same criticism could be made of Maury. It seems very likely, however, that most of the writers of the later nineteenth century simply did not consider that the motion of which Dove spoke was the general circulation. The notion that the general circulation meant the time-averaged or the time-and-longitude-averaged circulation had become rather well established, and Dove's currents varied with time and longitude. It is noteworthy that Hann (1901), in what is still one of the most comprehensive meteorological treatises yet produced, makes no mention of Dove in his chapter on the general circulation, but reviews Dove's ideas in detail in the following chapter on storms.

Yet despite the apparent rejection of Dove's scheme, the symmetric theories of the general circulation could not endure forever. Even as Ferrel and Thomson were making their final contributions, specific objections to them were being raised.

One of these was based mainly upon theoretical considerations. Except near the Earth's surface, friction was generally considered to be negligible. It was often pointed out that in the schemes of Ferrel and Thomson the poleward-moving air aloft would acquire unheard-of velocities in middle latitudes, in conserving its absolute angular momentum. Some writers took the attitude that such high velocities, never being observed, could not possibly exist, and that the schemes were dynamically impossible.

Others took the more moderate attitude that the high velocities simply did not exist, and that the schemes, while perhaps possible, were incorrect. In any event the thermal wind relation, adhered to by Ferrel, would not allow such high velocities to occur aloft unless excessively high velocities occurred at the Earth's surface also. The simple scheme of having the poleward current aloft so weak that there would be ample time for even weak friction to reduce the westerlies does not seem to have found favour.

In a remarkable paper the renowned physicist Hermann von Helmholtz (1888) attacked this problem. He had previously (1868) been the first to emphasize that the motion in a fluid need not be everywhere continuous. He began the present work by noting that friction was extremely ineffective in the atmosphere, except at the Earth's surface and at internal surfaces of discontinuity. Clearly, he was here referring to molecular friction. He then noted the large velocities which would be required by a circulation between the Equator and 30° latitude, and maintained that while such a circulation did occur, the large velocities were not found. He thereupon sought the means by which the winds were prevented from attaining them.

In seeking a solution Helmholtz virtually developed a theory of the general circulation. By reasoning somewhat like Ferrel's, he deduced that in the absence of friction there would be easterly winds in low latitudes and westerlies in higher latitudes. He then proceeded to determine how this circulation would be modified by heating and friction. Like Ferrel and Thomson, he found that surface friction would produce a poleward drift in the surface westerlies, and intensify the equatorward drift in the surface easterlies.

In his next step he proceeded beyond the earlier authors. He maintained that the returning air above the trades must come into immediate contact with the cooler and more slowly moving air below, with the formation of a surface of discontinuity. At such a surface the equilibrium would be unstable, so that vortices would form, and ultimately bring about vertical mixing.

In the polar regions he felt that the effect of cooling would outweigh the effect of surface friction, and lead to additional equatorward spreading. Easterly winds would thereby develop, and the resulting friction would cause further spreading. Again a surface of discontinuity would form between this air and the returning air aloft, and vertical mixing would again occur.

He concluded with the opinion that the principal deterrent to stronger winds aloft was not surface friction, but the mixing of layers of different velocities by means of vortices forming on surfaces of discontinuity.

Helmholtz's paper is often regarded as the original statement that cyclones must form upon surfaces of discontinuity, and that these cyclones will in turn alter the general circulation. Admittedly some statements are subject to more than one interpretation, but we do not feel that this is what Helmholtz was saying. The vortices which he visualized seem to have horizontal scales of hundreds of metres, or perhaps a few kilometres, but not thousands of kilometres, and he frequently mentioned billow clouds. He referred to cyclones in only two connexions, in neither case as disturbances on unstable surfaces of discontinuity. He first suggested that they should form in middle latitudes under the masses of ascending air. Later he clearly maintained that the permanent circumpolar anticyclonic motion at the Earth's surface and the cyclonic motion above it should break up into smaller cyclones and anticyclones as a result of surface irregularities, such as mountains. In neither case did he specifically say that these cyclones would affect the general circulation; it is the vortices which develop on the surfaces of discontinuity which were assigned this role. In a second paper (1889) he mentioned that the numerous disturbances should cause the principal surface of discontinuity to break "into separate pieces which must appear as cyclones," but he did not elaborate further.